Gerald Holton

The scientific imagination

Case studies

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Introduction

I

Considering the progress made in the sciences themselves over the past three centuries, it is remarkable how little consensus has developed on how the scientific imagination functions. Speculations concerning the processes by which the mind gathers truths about nature are among the oldest and still most prolific and controversial cognitive productions. Unless the inevitable distortion of near perspective is misleading me, it appears that only in the relatively recent period have proposals been made that have long-range promise.

The chief aim of this book is to contribute concepts and methods that will increase our understanding of the imagination of scientists engaged in the act of doing science. These chapters are therefore a continuation of the series of case studies which I published a few years ago in *Thematic Origins of Scientific Thought: Kepler to Einstein.*[1] As was the case there, my approach may be characterized by four aspects.

First, I try to make a detailed examination of the nascent phase of the scientist's work, and to juxtapose his published results, on the one hand, with firsthand documentation (correspondence, interviews, notebooks, etc.), on the other. In such a pursuit, one must be ready for the unexpected. Thus, in the studies on Einstein in my earlier book, the documents forced a reevaluation of the supposed genetic role of the Michelson experiment in Einstein's original formulation of relativity theory, and revealed that this role was small and indirect – contrary to the standard accounts and to the sequence given in practically all physics texts dealing with the matter (including a text I myself had published).

Similarly, in the new case studies presented here, the documents help us to account for the motivation of Fermi and his collaborators as we follow the historical development of the processes that led to another major scientific discovery, in this case, induced radioactivity by neutron bombardment. Both the study of how Robert A. Millikan dealt with the experimental data on which he based the published value of the electronic charge, and the comparison of his actual laboratory notebooks with his publications, require us to introduce a notion familiar from literary analysis but new to the analysis of scientific works. In my view, it is in the fine structure, in the detail of documented case studies, that one may hope to find the material with which to shape and test a theory of the scientific imagination – even if that task will not be completed easily or soon.

Second, I tend to look on any product of scientific work, whether published or not, as an "event" that stands at the intersection of certain historical trajectories, such as of the largely private or personal scientific activity; of the shared, "public" scientific knowledge of the larger community; of the sociological setting in which a science is being developed; and indeed of the cultural context of the time. In the previous book, I have described Niels Bohr's debt to works in philosophy and literature, and Einstein's interaction with epistemological currents. In this volume, I examined the effects of national resources and national styles on the organization and achievement of a laboratory team (Chapter 5).

Third, a particular concern in my studies is to find the extent to which, on certain crucial occasions, the imagination of a scientist may be guided by his, perhaps implicit, fidelity to one or more *themata.* Adherence to such preconceptions may help or impede the scientist; as Einstein once wrote to de Sitter, "Conviction is a good mainspring, but a bad regulator." The thematic structure of scientific work, one that can be thought of as largely independent of the empirical and analytical content, emerges from the study of the options that were in principle open to a scientist. It can play a dominant role in the initiation and acceptance of, or controversy over, scientific insights. Function and types of themata were discussed in detail in the case studies given in *Thematic Origins*, and are developed further here, in the cases to which the first half or so of these pages are explicitly devoted.

Last but not least, I am drawn to consider the practical consequences of such findings, for the development of scholarship in the history and philosophy of science, for the better understanding of the place of science in our culture, and for educational programs. This is the function of the last set of chapters.

II

Of these four related areas of chief interest, the topic of thematic analysis, although still an unfinished research subject, has perhaps the largest share of any claim to novelty. Judging from the commentaries on this work published in the past few years,[2] there may be several reasons for the interest that has been shown so far:

1. Thematic analysis allows discernment of some constancies or continuities in the development of science, of relatively stable structures that extend across supposed revolutions and among apparently incommensurable rival theories. Further, in this period of reactions against the philosophy that views science as a suprahistorical and culturally transcendent method of investigation, some scholars are attracted to the finding that a basic feature of the work of many seminal scientists is their acceptance of only a small number of themata, and that their debates frequently involve antithetical dyads or triplets of themata – for example, atomicity/continuum, simplicity/complexity, analysis/synthesis, constancy/evolution/catastrophic change. Such posits help to explain the formation of traditions or schools, and the course of controversies.

2. Although practically all my case studies so far have been concerned with the physical sciences, some results will be found to be applicable to the other sciences. There is evidence of this possibility, for example, with respect to recent studies in the history of biology,[3] in early biochemistry,[4] in sociology,[5] and in psychology.[6]

3. Techniques analogous to the thematic analysis that I have

applied to science have worked well before in other fields, for example, in content analysis, linguistic analysis, and cultural anthropology. It appears therefore that the work of mapping and classifying themata can lay bare basic commonalities between scientific and humanistic concerns that are not equally likely to become evident through other means. Thus in Harry Wolfson's *Philo*[7] the eloquent passage about the purpose of studying the work of the philosopher illuminates also the purpose of studying the work of the scientist in the mode here proposed. Wolfson wrote:

> No philosopher has ever given expression to the full content of his mind. Some of them tell us only part of it; some of them veil their thought underneath some artificial literary form; some of them philosophize as birds sing, without being aware that they are repeating ancient tunes. Words, in general, by the very limitation of their nature, conceal one's thought as much as they reveal it; and the uttered words of philosophers, at their best and fullest, are nothing but floating buoys which signal the presence of submerged, unuttered thoughts. The purpose of historical research and philosophy, therefore, is to uncover these unuttered thoughts, to reconstruct the latent processes of reasoning that always lie behind uttered words, and to try to determine the true meaning of what is said by tracing back the story of how it came to be said, and why it is said in the manner in which it is said.

4. The investigation of preconceptions in and concerning science connects rather directly with a number of other modern studies, including that of human cognition and perception, learning, motivation, and even career selection (as discussed in Chapter 7). Moreover, one may hope that a more sophisticated idea of the working rationality of scientists – with its full set of antithetical components, including preconceptions on the one hand and objective techniques on the other – will help to deflate foolish and dangerous ideas about science that, as noted in Chapter 3, have characterized some of the popular conceptions of science. As we are entering a period of an increasing number of externally imposed restrictions on and directions of scientific research, it is well for scientists and other scholars to ensure that

the conditions under which scientific originality can flourish are studied, more widely understood, and protected.

III

The deep attachment of some scientists to certain overarching themata may well be one of the chief sources of innovative energy (parallel to that of the instrumentalist or utilitarian thrust in science). It seems to me otherwise difficult to understand a key fact about the sciences, namely, that again and again they have been regarded as verging on a charismatic activity rather than being thought of as, say, merely one of the more successful but fundamentally pedestrian activities of mankind.

To underline, and at least briefly emphasize this sometimes neglected point, we cannot do better than reflect on a work standing at the very beginning of modern science. Copernicus, like any other good astronomer, relied of course on observation and calculation, and he greatly advanced mathematical astronomy in the technical sense. But one must look deeper to find the chief reason why he came to write the work for which he has been honored, or to explain its power. Nature, he held, is God's temple, and he implied that human beings can, through the study of nature, discern directly both the reality and the design of the creator. This was a daring and dangerous idea, and it is significant that when Copernicus's book was put on the *Index* of "Books to be Corrected," this implication was one of the relatively few deletions which were insisted upon as necessary; for it was clear both to Copernicus and to his opponents that when the purpose of science is perceived large enough, it can rival the claims of all other reality systems.

From the first sentence of *De Revolutionibus*, one senses the source of energy of a major scientific idea. It is not some pedestrian piecing together of a corner of the puzzle. Nor does the work give us merely better astrometry and applications such as calendar corrections, valuable though these are. Rather, his discovery is on a scale that produces an expansion of human consciousness, a change in cultural evolution – and it was so perceived by those who were converted to Copernicus's idea.

In his work, two themata predominate, and the mutual accommodation of theory and data that they produce seem to me to account for the quasi-aesthetic conviction in his followers that the system must be right. These themata are those of simplicity and necessity. They appear in a stern manner that became basic to all science since. In a well-known passage, Copernicus proudly writes that the heliocentric scheme he has found for the system of planets has the property that "not only their phenomena follow therefrom, but also the order and size of all the planets and spheres and heaven itself are so linked together that in no portion of it can anything be shifted without disrupting the remaining parts and the universe as a whole."

The power of this solution was precisely its restrictiveness. There is nothing arbitrary, no room for the smallest ad hoc rearrangement of any orbit, as had been quite possible in pre-Copernican work. Copernicus's system, as a whole, revealed a sparse rationale, a necessity that binds each detail to the whole design. Hence it carries the conviction that we understand why the planets are disposed as they are, and not otherwise. One is reminded here again of Einstein's remark to his assistant Ernst Straus: "What I'm really interested in is whether God could have made the world in a different way; that is, whether the necessity of logical simplicity leaves any freedom at all."

This kind of terminology, and the attitude behind it, are now rare and even somewhat embarrassing to most scientists. There are good sociological, psychological, and even political reasons why this should be so, why our usual list of motivations for scientific work tends to stress the Baconian side of the heritage of modern science – the discovery of cures, the perfection of machinery, the strengthening of the state's security, or simply the provision of a decent way to spend one's days on this earth. But while the Baconian ethos has become a necessary component of the total scientific and engineering enterprise, it would not be sufficient to sustain science, and by itself does not help us understand the nature of high discovery.

No one would argue that personal testimonies such as those referred to should be introduced into our current scientific papers. However, a quiet underground current of this cosmological tradition still exists. It comes in a somewhat disguised form, but the thematic content of simplicity and necessity as warrants of deeper truths are still among the most prized.[8] Steven Weinberg, on receiving the Robert Oppenheimer Memorial Prize, said:

> Different physicists have different motivations, and I can only speak with certainty about my own. To me, the reason for spending so much effort and money on elementary particle research is not that particles are so interesting in themselves – if I wanted a perfect image of tedium, one million bubble chamber photographs would do very well – but rather that as far as we can tell, it is in the area of elementary particles and fields (and perhaps also of cosmology) that we will find the ultimate laws of nature, the few simple general principles which determine why all of nature is the way it is . . .
>
> The reason I take such an optimistic view of where we are now is that relativity and quantum mechanics, taken together but without any additional assumptions, are extraordinarily restrictive principles. Quantum mechanics without relativity would allow us to conceive of a great many possible physical systems. Open any textbook on non-relativistic quantum mechanics and you will find a rich variety of made-up examples – particles in rigid boxes, particles on springs, and so on – which do not exist in the real world but are perfectly consistent with the principles of quantum mechanics. However, when you put quantum mechanics together with relativity, you find that it is nearly impossible to conceive of any possible physical systems at all. Nature somehow manages to be both relativistic and quantum mechanical; but those two requirements restrict it so much that it has only a limited choice of how to be – hopefully a very limited choice.[9]

All scientists since Copernicus have understood the attractiveness of a system having such qualities. And although any individual attempt of this sort is an act of intellectual and professional risk taking – for the thematic choices themselves are neither verifiable nor falsifiable, and the antithetical thema of complexity, for example, deserves a more detailed analysis than it has been given so far – no other, less cosmological, approach is likely to lead to the truth, least of all to a truth having the exalting sweep that historically has helped give the scientific enterprise its intellectual mandate.

IV

In pointing to these uses to which themata have been put, I do not mean to imply that they are the only ones of significance. On the contrary, themata have been and, I expect, ever will be used by scientists of the most opposing attitudes and interests. Moreover, I do not believe that giving attention to thematic analysis requires one to adopt a label (certainly neither "positivistic" nor "antipositivistic"), or otherwise forces one to take sides in the battles currently preoccupying some sectors in the history and philosophy of science – the more so as many of the divisions are themselves along thematically opposing conceptions about the history or philosophy of science. The campaign flags, or accusations, read "objectivity" versus "subjectivity" versus "anything goes"; "logical" versus "empirical" versus "psychologistic" studies; "rules of reason" versus "mystical conversion"; "rational" versus "irrational"; "relativism" versus "absolutism"; "analytical-reductionistic" versus "holistic"; and even "reason" versus "imagination." But to paraphrase a seminal paper that changed the state of physics in the early years of this century, the understanding of the process of scientific innovation that is implied in these antagonistic schemes is characterized by polarities which do not appear to be inherent in the phenomena, that is, in the actual work of the scientists as it reveals itself to us in archival material, oral histories, and of course in the actual participation in research.

At the very least, the present, opposing positions seem to me to lack the flexibility and the ability to accommodate themselves to the human activities – with all their natural ambiguities – that we are trying to map and study. The heat and ideological clamor emanating from some of the encounters in fields that make science their raw materials of observation are strangely incongruous with respect to the state of affairs in the sciences themselves. Ironically, even those who actually work in the "hardest" sciences now are often satisfied with claiming no more than "good reasons" and probable knowledge. Most of them are not afraid to accept humanistic interpretations of their work, and are likely to sympathize with Henry A. Murray's perceptive definition that "science is the creative product of an engagement between the scientist and the events to which he is attentive."[10]

V

The search for models of the scientific imagination, at this stage of research, must of necessity be largely inductive and empirical. It must be committed to painstaking attempts at historical accuracy and cautious scholarship based on the available evidence, but it must also possess the imaginative freedom to produce new conceptual tools with which to study well-guarded areas such as the working of the minds of scientists. Adopting a kind of ethological approach to the study of scientific activity and bringing in whatever is needed – now the state of science as understood at the time, now findings on the psychodynamics of scientists or the social forces on them – seem to me a strategy preferable to casting the accounts of achievements into formalistic structures. As we are only just beginning to gather the chief elements from which theories of the scientific imagination may be fashioned, schemes that promise certainties must be held at arm's length.

One recalls here a story told by the architect LeCorbusier.[11] Having invented his "modulor," a measure-system for fixing the dimensions of architectural space, of urban design, and of plastic arts, and believing in its necessity and power with fervor, he was arguing intensely for its wide adoption. LeCorbusier even journeyed to Princeton to convince Einstein. However, instead of the hoped-for endorsement of the system, he obtained a much milder and more appropriate judgment: Einstein told him the scheme would be quite satisfactory if it only served to make the bad more difficult, and the good easier.

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CONTENTS

Part I: On the thematic analysis of science